

to accord my hearty agreement with his timely protest against the advertising of a chair of chemistry at King's College, London, at the "princely salary" of 600*l.* a year.

Prof. Armstrong hopes that the profession will make no response. There is, however, little doubt that there will be a quite substantial competition for the post among the best of our chemical lecturers. The reason is, of course, that 600*l.* does appear to the university lecturer a princely plum worth scrambling for; it means for him at one jump an increase to from two to three times his present salary, and he knows that as the number of such relatively well-paid posts is much smaller than the number of lecturers, it is his duty to his dependents to leave no chance untried.

That I am not drawing an exaggerated picture will be evident from a consideration of the following average salaries of non-professorial teachers, calculated from data which I have before me, derived from fifteen universities and university colleges:—

Service year	Number of lecturers	Average salary <i>£</i>
1-5 ...	119	171
6-10 ...	85	193
11-15 ...	77	253
16-20 ...	30	239
21-25 ...	11	263
26-30 ...	4	297
31-35 ...	3	273

The professorial view of a salary of 600*l.* is another matter. What the gentleman appointed to the chair at King's College will think a few years hence is also another matter.

The preliminary to any effort to maintain a reasonable professorial scale of remuneration by the abstention of lecturers from competing for what are for them really well-paid posts is to ensure the lecturer a reasonable living wage. And this, too, is Prof. Armstrong's solution put in rather different form. Why not call the post a lectureship? A glance at the above table will show that to abstain from competing for a 600*l.* post, simply because it is called a professorship, in, say, one's twelfth year of service, is a risk too grave to be taken.

The Scottish lecturers have been recently granted a graded scale rising to 750*l.* This is in line with the revised scheme of remuneration for the scientific staff of the National Physical Laboratory, as follows:—

	Min. <i>£</i>	Yearly increase <i>£</i>	Max. <i>£</i>
Superintendents ...	800	50	1000
Principal assistants ...	650	25	750
Senior assistants ...	500	25	600
Assistants, I. ...	350	20	450
Assistants, II. ...	250	20	350
Junior assistants ...	175	15	235

In regard to abstention, another and very vital difficulty arises. How is a lecturer to know what his colleagues will do? He may by abstaining cut his own throat without achieving any reform. Both professors and lecturers are in a dilemma. What is to be done?

The answer is perfectly clear. We university teachers should have an association which would do for us, or enable us to do for ourselves, what the Medical Association does for the doctors. This is a perfectly practical suggestion. It may be taken as certain that in the course of evolution such an association is destined to come. What is to prevent it from being an accomplished fact within six months from to-day?

R. DOUGLAS LAURIE.

University College of Wales, Aberystwyth,
January 9.

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Airy and the Figure of the Earth.

I SHOULD be very much obliged if any reader of NATURE would kindly give me an answer to the following question:—

In a note on p. 371 of the second volume of the treatise on "Natural Philosophy," by Thomson and Tait, I read: "Airy has estimated 24 ft. as the greatest deviation of the bounding surface from the true ellipsoid." Where did Airy give the result 24 ft.?

Darwin ("Scientific Papers," vol. iii., p. 78) writes: "Airy further concluded that the earth's surface must be depressed below the level of the true ellipsoid in middle latitudes. He gave no numerical estimate of this depression, but expressed the opinion that it must be very small."

It is curious to remark that Todhunter, in his "History of the Theory of Attraction, etc.," made no mention of the paper by Airy on the figure of the earth printed in the third part of the Philosophical Transactions of the Royal Society for the year 1826, which is the paper alluded to in the above quotation from Darwin.

OTTAVIO ZANOTTI BIANCO

(Docent of Geodesy in the R. University of Turin).

Torino (Italy), Via della Rocca 28,
November 28, 1918.

I HAVE carefully examined the curious points raised by Sig. Bianco, and cannot find any satisfactory answer to the questions asked.

Thomson and Tait's footnote, in which it is stated that Airy "estimated 24 ft. as the greatest deviation of the bounding surface from the true ellipsoid," occurs also in the first edition of the "Natural Philosophy." It is true, in a sense, that Airy gave in his published paper no distinct numerical estimate of this deviation. Nevertheless, in his discussion of Sabine's pendulum observations, he compared the coefficients of certain terms with the corresponding values in terms of e and A as given in his theoretical formulæ. The value of A so found (see Phil. Trans., vol. cxvi., p. 366) is 0.000064. In latitude 45° the deviation is $aA \times \frac{1}{2} \times \frac{1}{2}$, and this is 334 ft., and not 24 ft., as Thomson and Tait give it.

The only other possible explanation is that Airy had communicated a later estimate privately to Thomson, for it is quite conceivable that Airy may have made an estimate which he never published. I can find no other publication of his in which this estimate is given or from which it may be derived.

In the days in which Thomson and Tait were writing their book—i.e. in the "sixties" of last century—Thomson was much interested in the figure of the earth, and he was almost certain to be in touch with Airy.

Personally, I never thought of questioning the accuracy of the footnote. Sir George Darwin read the proof-sheets of the second edition and wrote a number of sections. He let it pass, although in his own writings he says that Airy gave no estimate. This, however, is not quite correct.

I can throw no further light on it. Possibly a better series of pendulum observations than those given by Sabine might lead to the result 24 ft. But that is rather a wild speculation.

C. G. KNOTT.

Some Temperature Anomalies.

I HAVE often noticed the anomalies of temperature to which Mr. Harries directs attention in NATURE of January 9, and have sometimes been inclined to ascribe the high temperature in the east to air that

has come from Spain and Africa instead of from the Atlantic, as in the west. But as the result of observations of temperature in the upper air I have latterly thought that Mr. Harries's suggestion is correct, and that the high temperature is due to a descending current. So far as my recollection goes, the phenomenon occurs when an anticyclone is situated over France or the south-east of England, and not in cyclonic conditions such as have prevailed during the past week.

Whatever the cause may be, the temperatures and pressures of the air from 2000 ft. to 25,000 ft. are most highly correlated; from 10,000 ft. to 20,000 ft. the correlation coefficients between temperatures and pressures at the same height are as high as 0.80 to 0.90, and even at 2000 ft. the temperature is far more dependent upon the pressure at 30,000 ft. than it is upon the direction of the wind. Above 35,000 ft. the correlation is negative. It seems pretty clear that this close connection between temperature and pressure must be due to vertical currents induced by the distribution of pressure; it is too close, and above 30,000 ft. of the wrong sign, to be accounted for by the mere adiabatic compression and expansion without change of height, and it may well be that on some occasions the descending currents reach the surface and produce a high temperature, although in general the temperature at the surface is not much influenced by the pressure.

W. H. DINES.

Benson, January 10.

Cyclones.

"J. S. D." in his interesting article in NATURE for January 2 last, makes the following statement:—"Thus the cyclone was looked upon as a warm column of rising air with spirally inflowing winds at its base; the anticyclone, conversely, contained a cold core of descending air. Now we know that the opposite is in reality the truth; the cyclone has a cold core, the anticyclone a warm one." I think it should be pointed out that this pronouncement is only correct for the troposphere, but not for the stratosphere.

Modern methods of sounding the atmosphere have shown that the Arctic and Antarctic cyclones have warm centres in the stratosphere, and Dines (Met. Office Pub., 210b, p. 50) shows that this is true of travelling cyclones also.

Too much importance has been attached to the temperature distribution in the lower portion of the troposphere. Modern discoveries have merely located the hot core of the cyclone in the stratosphere instead of in the troposphere, leaving the temperature theory still the cause of the cyclonic circulation of the wind and the force that lifts up a cool central column of air from the ground.

I think it will be found that the energy of cyclones can be maintained on the temperature theory with a very slight interchange of air between the stratosphere and troposphere in the case of the polar cyclones, and in the case of travelling cyclones by the bodily rising of the air in the central regions during their comparatively brief life.

R. MOUNTFORD DEELEY.

25 Beaconsfield Villas, Brighton, January 4.

MR. R. M. DEELEY is quite right in pointing out that it is only in the troposphere that depressions are relatively cold and anticyclones warm. It is in this region that the striking contrast appears between the old preconceived theory which postulated a warm core and the results of modern observation. The mechanism by which a cyclonic depression is maintained in being

forms one of the great unsolved problems in meteorology. Some years ago the suggestion was put forward by Mr. W. H. Dines that the driving force of the depression was to be looked for in the level at the base of the stratosphere. According to this view, a very slowly descending, and therefore warmed, column of air in the stratosphere is just such an integral part of the whole system as the rising, and therefore cold, column in the troposphere, but neither the one nor the other is to be regarded as the cause of the depression. Mr. Deeley may be right in his view that the warm column in the upper layers is the fundamental cause, but this view is not at present generally accepted.

J. S. D.

The Brussels Natural History Museum.

To many of your readers who appreciate the value of the collections in the Brussels Museum, the following extract from a letter written by Dr. Dollo on January 5 will be welcome news:—"Mais je vous avais écrit également une carte postale illustrée, représentant notre Galerie des Vertébrés vivants et fossiles de la Belgique, pour vous dire que tout était bien ici, que notre Musée est intact, qu'il n'y manque absolument rien, et que nous étions saufs!"

A. C. SEWARD.

Downing College Lodge, Cambridge,
January 12.

BORINGS FOR OIL IN THE UNITED KINGDOM.

THERE is no need to labour the importance of liquid fuel in our national economy and existence. The growing needs of our Navy and Air Service, and the difficulties of transport during the war, have driven home the lesson and rendered imperative the demand that we should increase to a maximum the output of liquid fuels in the British Isles. The present production, mainly from the Scotch oil-shales and some of the coal-tar distillates, is very inadequate, and the country has had to depend almost entirely on foreign supplies. Such a state of affairs is obviously deplorable, and if remedies are possible the neglect to apply them would be highly culpable. Two methods of alleviating the situation have been suggested, and both are being tried. The first entails the extensive retorting of British oil-shales and cannels to produce oil by destructive distillation; the second involves the drilling of wells in selected areas in a search for free crude petroleum in commercial quantities. With the first method the present writer is not here concerned; it is the attempt to find oil in the free state which forms the subject of the ensuing remarks.

The generally received opinion, that Nature, so lavish in her gifts of coal and iron to these favoured islands, was unaccountably frugal with petroleum, has not been accepted by all. A small minority has urged, and recently with insistence, that the assumption is based largely on the absence of definite intelligent exploratory drilling—that we are, in fact, in the same position as the United States before the Drake well of 1859, ignorant of the great stores of wealth lying available below the surface.

If this view be sound its importance cannot be